VIEWPOINT

Is there a Prehistory of Linguistics?

with contributions from
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Viewpoint

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There are few aspects of human behaviour more fundamental than our ability to use language. Language plays a key role in the study of any living human society, and of all historical communities which have left us written records. In theory it could also throw enormous light on the development and relationships of prehistoric human communities. But here there is a huge and obvious problem: what evidence can there be for human languages in the pre-literate, prehistoric age? In other words, what hope is there for a prehistory of linguistics? There is no easy answer, yet it is hard to accept that any account of human prehistory can be considered adequate without some knowledge of prehistoric languages and linguistic relationships, if only at the broadest scale.

The list of questions we might wish to pose stretches back to the period of the very earliest hominids. When did our human ancestors first begin to talk to each other? Was language acquisition sudden or gradual? Did human language arise in one place, and then spread and diversify from that point? Or did it emerge independently, among separate groups of early humans in different parts of the world?

Leading on from this is the study of ethnicity and ethnogenesis. Since the end of the nineteenth century one of the biggest problems facing prehistoric archaeologists has been the identification and interpretation of archaeological cultures and cultural groups. Do these have any social or ethnic reality? Is it right to speak of a Beaker 'folk'? Was the Bandkeramik colonization the work of one people or of many? These questions would be so much easier to resolve if only we could trace the prehistory of languages, and could establish, for instance, whether all Bandkeramik and Beaker users spoke the same or a related language.

Such possibilities may seem exciting and hopeful to some, irredeemably optimistic to others. Whatever view we take, they clearly merit serious discussion. In the present Viewpoint, our third in the series, we have asked five writers — two archaeologists (Renfrew & Bellwood), three linguists (Bynon, Ruhlen & Dolgopolsky) — to give their own, personal response to the key question 'Is there a prehistory of linguistics?' Can we, from the evidence of archaeology, linguistics (and now DNA studies), say anything positive about language in prehistory?

Towards a New Synthesis?

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Recently the relationship between historical linguistics and archaeology has come under increasing discussion after decades of relative neglect. Several factors have converged to generate new interest and indeed (in some quarters) new optimism. Among these are recent developments in the fields of historical linguistics, of molecular genetics and indeed of prehistoric archaeology. This has led to suggestions that a 'new synthesis' between these disciplines is in process of formation (Renfrew 1991; Ruhlen 1991, 397).

It is important to recognize, however, that many historical linguists — perhaps the majority — feel that this optimism is misplaced and that nothing very meaningful can be said about early languages prior to about 4000 BC. These scholars would argue that the well-tried methods of linguistic reconstruction — the 'comparative method', originally developed in relation to the Indo-European languages and to the reconstruction of Proto-Indo-European — do indeed allow statements to be made about early languages prior to their documentation in written form. But they see a well-defined limit to the time depth for which this procedure is valid. And they see attempts to find deeper and earlier links between language families through the recognition of wider and more embracing 'macrofamilies' to be methodologically questionable, or even unsound (Morpurgo-Davies 1989; Campbell 1988).

The dilemma here is a very considerable one. For to many archaeologists it is clear that the existence of very extensive language families has to be the product of social and demographic processes in the past which in favourable circumstances should show up in the archaeological record. Such has certainly been claimed, for instance, for the speakers of the Polynesian languages, or the Bantu languages of Africa. The same is obvious also for the very much more recent dissemination of European languages in the Americas, in Africa and in the Pacific, over the past four centuries. Molecular biologists, using gene frequency studies and DNA sequencing, feel able to make very specific statements about early human population histories, which should have a direct impact upon our understanding of the origins of various language families. It is now possible to outline some of the avenues for exploration. Cavalli-Sforza and his colleagues have written informatively about links between new genetic data and early languages (1988). Peter Bellwood has been among those archaeologists seeking to discern the broad picture (Bellwood in press c; see Renfrew in press; 1992b). And there are at least two schools of historical linguists who feel able to penetrate the notional 4000 BC barrier in traditional historical linguistics, and to speak of deeper and more embracing linguistic phyla or macrofamilies.

But against these optimists, sometimes characterized as 'lumpers', who feel able to see supposed broad linguistic affinities uniting well-recognized language families into larger linguistic units, stand those more cautious and perhaps more rigorous scholars (the 'splitters') who are critical of attempts to postulate larger taxonomic units without prior and very thorough-going linguistic reconstruction.

Historical implications

So far, it seems to me, as an amateur in these linguistic debates, that the discipline of historical linguistics has divided itself into two fiercely opposed camps without developing any very well defined criteria for evaluating the differences between them. The theoretical issue centres upon the validity of the concept of the linguistic macrofamily, and on the methods properly to be used to postulate specific macrofamilies and to establish which particular languages may securely be regarded as belonging to them.

Yet, even working within the familial classifications accepted by nearly all linguists, there are historical and archaeological questions which cry out to be answered. The case of the Indo-European language family is the best known, and perhaps the most energetically discussed. But the circumstance that many of the arguments are familiar should not obscure the momentous nature of the implications for the study of European prehistory. If Marija Gimbutas really were correct, and the first appearance of Indo-European speakers in Europe came with her 'kurgan' invasions (see Mallory 1989) then her arguments might well be plausible that significant social transformations came about at the same time: the male-dominated warrior chiefdoms which she identified in the European Bronze Age could well be seen as consequences of the nomadic kurgan invasions, and the allegedly tripartite organization of society claimed by Dumézil would indeed be of fundamentally Indo-European character, from the Garonne to the Ganges. Within this frame of reference

the 'Coming of the Greeks' during the Aegean Bronze Age would be responsible for many of the particularities of Mycenaean civilization, and the 'Coming of the Celts' during the west European Iron Age for the transformations seen in the later first millennium BC. I have argued that these changes — or rather the changes observed (which do not have to be described in these terms) — may be regarded as internally motivated within a continent in which for some time languages ancestral to the later Indo-European languages had been spoken (Renfrew 1987). Many of the other language families of the world offer similar problems with vast implications. The Afroasiatic languages are a case in point. Linguists in general are agreed that this very large language family, which includes all the Semitic languages as well as Ancient Egyptian, the Berber languages and the Cushitic, Omotic and Chadic languages, represents a valid taxonomic unit, and that these families are probably descended from an ancestral proto-language. Yet such a conclusion should have major implications for the study of the prehistory of North Africa and the Near East. If a migrationist argument is proposed, the social and demographic forces needed to have an impact over so vast an area must indeed have been significant ones. And if a more static model of interaction and convergence be preferred, how on earth does one construct an explanation which can operate over such considerable tracts of land? The difficulty in formulating plausible explanations and then in testing or further investigating them has very reasonably inhibited discussion of these questions. But the increasing impact of molecular genetic studies upon our understanding of early population histories begins to offer the hope of resolving some questions. The answers cannot be other than of crucial significance for the understanding of the prehistory of these areas. Similar observations could be made for the language families in many parts of the world. And there is nothing linguistically controversial in the recognition of these language families, although the archaeological interpretations may leave room for vigorous argumentation.

Language families and their extent

When one studies the world distribution of language families (and here the discussion can be restricted to those families which are recognized as such by the great majority of linguists, so that the matter need no be regarded as controversial) one sees a striking pattern in the geographic extent of such families.

Some, such as Indo-European, or Afroasiatic, or Dravidian, or Austronesian, or (in Australia) the Pama-Nyungan family, occupy vast tracts of land. The areal extent taken up by the population of each individual language within the family is itself often a considerable one. On the other hand there are families with much more restricted distributions, like the North Caucasian languages, or the Indo-Pacific languages of New Guinea, or the languages of North Australia. The languages within these families are often less obviously closely related: if there is a common proto-language ancestral to the entire family it probably goes back to an earlier date. And the areal extent occupied by the speakers of each language within the family is often very much more limited. The linguist Johanna Nichols has distinguished these two classes of language family as occupying respectively 'spread zones' and 'residual zones' (Nichols 1992, 13), and the distinction seems a clear one.

Indeed it is possible to offer a historical explanation for the distinction (Renfrew in press). The language families in the former category have a distribution which is likely to be the product of a process of relatively recent language replacement over a very wide area. They are indeed the result of a *spread* process, whose mechanism requires to be further examined. On the other hand the language families occupying 'residual zones' — which are in many cases located in mountainous areas somewhat isolated geographically — may in some cases be regarded as *residual*. They have probably occupied the same geographical areas for much longer periods, hence their greater internal diversity. In some cases they may even be the result of the initial colonization of those territories by the first human populations to occupy them.

Here we do need to reckon with questions of time depth. And it is at this point, and only here, that I would turn away from the generally held linguistic view by proposing a much earlier time scale than would generally be accepted. The language families in the first, 'spread', category I would see as the result of post-Pleistocene dispersal processes, in many cases to be associated with the first spreads of farming economies in different parts of the world. The families in the second, 'residual' category have, I would argue, been in place for a very much longer time, stretching back into the Pleistocene and perhaps to the earliest human colonization of the areas in question.

In laying emphasis upon dispersal processes for the first group of language families, and in particular upon farming dispersals, I am very much in harmony with the views expressed by Peter

Bellwood in an influential series of papers (Bellwood 1991a; 1991b; 1994; below pp. 271–4). And in this case the evidence of molecular genetics can be of assistance. For dispersal processes of this kind do carry with them implications for gene frequencies which can be tested, and in many cases have been. The genetic evidence for the spread of farming in Europe has been extensively discussed by Cavalli-Sforza and his colleagues (1994, 255–301) and has found support on statistical grounds (Sokal *et al.* 1991). It can be expected, therefore, that there will be a steady flow of molecular genetic data which will have a bearing upon such questions, even if, by its nature, it is informative about questions of population rather than directly about early languages.

Macrofamilies

Two principal schools in the field of historical linguistics have developed the notion of the macrofamily or linguistic phylum. The Nostratic macrofamily was postulated by scholars originally working independently in Moscow: the late V.M. Illich-Svitych (1990) and Aron Dolgopolsky (1989, below pp. 268–71), now based in Israel. Using well established procedures in historical linguistics, in effect the comparative method, they have shown that the Indo-European, Uralic, Altaic, Kartvelian, Dravidian and Afroasiatic language families are related, and may be considered to belong to a larger macrofamily, termed Nostratic. Sergei Starostin (1989) has compared the North Caucasian, Yeniseian and Sino-Tibetan families in a similar manner, suggesting a further macrofamily. If these suggestions prove warranted, they would have very substantial archaeological implications, since in each case there is postulated a proto-language (e.g. Proto-Nostratic) seen as ancestral to the proto-languages of the individual families. This in turn may be taken to suggest a human population at some specific place and time speaking the Nostratic proto-language, and ancestral, presumably, to the speakers of the proto-languages of the individual families.

These are sweeping proposals, going back in time well beyond 10,000 years ago, far across the time barriers accepted by many historical linguists.

All of this should give rise to strong archaeological predictions, and to predictions also in the field of population genetics, which should be open to examination through molecular genetic studies.

The second approach to broader, more embracing macrofamilies has been led in the United States by Joseph Greenberg (1963; 1987) whose work in

Africa is now widely accepted, while that for the Americas has been the subject of fierce controversy. Merritt Ruhlen (below pp. 265–8) has been one of the principal advocates of the Greenbergian approach, which differs in some respects from that of the Nostratic school. Once again, the implications for prehistory would appear to be immense, were Greenberg's linguistic classifications to be accepted as meaningful and historically valid. For the historical processes underlying the dissemination of the various languages falling within one of Greenberg's macrofamilies might imply population dispersal processes on a very significant scale. The 'new synthesis' mentioned above involves finding archaeological correlates for these dispersal processes which may in turn find support from the available molecular genetic evidence.

Ruhlen goes further than the recognition of such macrofamilies, itself too bold a step for many historical linguists. He actually finds points of resemblance *between* the various macrofamilies, implying for him a still deeper level of linguistic unity which he sees as embracing potentially *all* the living languages of the world. They would thus all spring from a common source: a vision of linguistic monogenesis. It is of course tempting to relate this to the accumulating evidence for a single origin for our species some 100,000 years ago. But for the present I find this (as do most linguists) rather too much to cope with, and perhaps too easy to criticize. It would be a pity if the very substantial case for macrofamilies made by the Nostratic school, by Greenberg and by Ruhlen itself, and the very wide-ranging implications which they would carry for prehistoric archaeology, were to be called into question by these more speculative considerations.

Setting aside, for the moment, questions of monogenesis, the matter of the status of the macrofamily as a valid taxonomic category is one of great urgency for historical linguistics. The implications of such macrofamilies, should their validity be accepted, for the understanding of prehistory would be enormous. It is therefore to be regretted that the debates within the field of historical linguistics upon this topic have been so few and so inconclusive. Both sides could well be accused of dogmatism, and the dogmas in question are unfortunately contradictory and perhaps unreconcileable.

The promise of molecular genetics

Many of these matters might appear too speculative, too hypothetical, and entirely incapable of resolu-

tion, were it not for current progress in the field of molecular genetics. It seems at first sight remarkable that the comparison of genetic information from different *living* populations should actually be capable of giving information about their history and descent, but such indeed proves to be the case. One can indeed contribute to the understanding of relationships between individuals and populations in this way. It should be clearly understood that this in itself tells us nothing about the languages spoken by those populations or by their ancestors. But as indicated above, most historical explanations in the field of historical linguistics involve the consideration of migrations and dispersals, of population growth and of other demographic phenomena. Such matters can be illuminated through the methods of molecular genetics, even if these can say nothing in themselves directly about early languages.

It is in the undoubtedly difficult area, from the theoretical standpoint, between these three disciplines of prehistoric archaeology, historical linguistics and molecular biology, that real progress is now on the way. For that reason it may not be unduly optimistic to answer the initial question here — 'Is there a prehistory of linguistics ?' — in the affirmative.

Can there Ever be a Prehistorical Linguistics?

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Comparative-historical linguistics is, in the first instance, concerned with investigating the mechanics and the explanation of language change. It seeks to determine, for instance, how and why languages change their basic word order, to what extent their morphology reflects earlier syntax, what features of a language may be 'borrowed' from another language. The primary data for such investigation consist of texts from successive stages in the history of a single language, or material obtained from native speaker informants of related languages and or languages assumed to be related.

A 'prehistorical linguistics' will thus only be possible in so far as the methods employed, the 'tools of our trade', permit us to penetrate beyond the time-depth of our sources. The further step of relating 'reconstructed languages' to the archaeological record is one which ventures beyond the strict confines of our discipline and takes us into a no-man's-land fraught with pitfalls. The silence or even critical reaction on the part of some linguists (for instance, Szemerényi 1989; Polomé 1991) to Renfrew's proposed synthesis of archaeological and linguistic evidence (1987) should not therefore be taken as indicating that the matter is considered to be of no interest. The topic in fact very much captures the imagination of the historical linguist. This was demonstrated by the unusually large audience attending the meeting of the Philological Society devoted to it (Renfrew 1989). The reasons for our caution lie deeper and have to do rather with the limitations of the methods of investigation at our disposal.

The comparative method is the most powerful of these. It enables us, for a family of related languages, to construct not, as is often believed, a proto-language, but at best an incomplete 'initial' grammar and lexicon, together with an ordered set of reanalyses ('changes') by which it is linked to each language in the family. This allows us to explain such things as why the plural of *goose* is *geese*, how *goose* relates to *gander*, and *bring* to *brought*. Linguistic 'reconstruction' thus does not give any sort of direct access to a lost language, but where we can test our methods (as in the case of the Romance languages where we have the control of Latin) the construct may be accepted as at least an idealized representation of a real language. But of course it must be remembered that it is not this construct but the underlying real language which it reflects (never completely and often very imperfectly) that has a location in time and space.

A second kind of language grouping which has historical implications is the Linguistic Area or *Sprachbund* (literally 'language league'). This can be defined as a group of geographically contiguous languages having shared features which are not inherited from a common ancestor but have been acquired as the result of prolonged bilingualism. When two or more languages are in intimate contact within the mind of a bilingual speaker, particularly when this is an adult, interference phenomena are bound to occur. In the case where the whole population or a large section of it is bilingual this will, in the long run, result in some degree of linguistic convergence. Thus, while the language family reflects linguistic divergence from a common ancestor, the *Sprachbund* reflects linguistic convergence due to geographical contact. Trubetzkoy (1928) proposed a set of criteria (Fig. 1) for differentiating between the two.

In Trubetzkoy's characterization of these two language groupings, it is the preponderance of *shared substance* (cognate basic vocabulary, cognate morphological elements, systematic sound correspondences) in the language family compared with that of *structural isomorphisms* in the linguistic area which is significant. Shared culture words are in fact a frequent but not a necessary concomitant of areal convergence. In practice, the two types of grouping often overlap. Bulgarian, for instance, is a member of both the Slavonic language family and of the Balkans *Sprachbund*. In its case the historical and comparative evidence clearly demonstrates that the local innovations which it shares with neighbouring Balkan languages are later than what unites it with its historical relatives outside the Balkans area. In a similar way it has been proposed that the aboriginal languages of Australia form both a language family and a linguistic area (Dixon in press). Again, Gamkrelidze & Ivanov (1995) propose locating the parent languages of Indo-European, Kartvelian and Semitic within a single linguistic area on the strength of early loanwords and of at least one basic structural isomorphism, namely that all three proto-languages shared a three-term phonological opposition of voiced, voiceless and glottalized consonants (the so-called 'glottalic hypothesis'). This is still controversial in the case of Indo-European (see Hayward 1989 for critical comment). They also postulate a 'secondary IE homeland' north of the Black Sea in a convergence area formed by the later European languages, excluding Greek (see below).

Shared structure cannot, however, on its own be taken as diagnostic of either areal convergence or of common inheritance. This is because of the very limited overall range of structural possibilities available. The chances of the three constituents — subject, verb, object — in such a sentence as *The dog chased the cat* having this particular order are in fact quite high. If we accept that the categories subject (S), verb (V) and object (O) are universals, in the sense that they are required in the grammatical description of any language, then cross-linguistic diversity (or *variation*) must be confined, where these three categories are concerned, to their relative ordering. The total theoretical possibilities are six: SVO, SOV, VSO, VOS, OSV and OVS. In practice, of these six possibilities, only the first three are found widely throughout the languages of the world, the last three being exceedingly rare. The chances of any two or three languages having the same ordering of these basic constituents without there being any historical connection between them is therefore statistically quite high. Furthermore it has been found that there is a significant degree of correlation between (S)OV order and a noun phrase structure of the type *black dog* or *John's dog*, and again between VO (SVO,VSO) order and a noun phrase pattern *dog black* or *dog of John*. We have here entered the, at present, rapidly developing domain of language typology and linguistic universals (Comrie 1989).

Sometimes, however, specific word order types do cluster geographically. Thus there is a huge OV area ranging from Turkey to Japan, and a VSO area spanning North Africa and the extreme western seaboard of Europe (the area of the megaliths). It seems reasonable to suppose that such marked deviations from the expected random distribution of typological features in geographical space could have prehistoric implications. So also could such other large-scale areal features as lexical tone and isolating structure in East Asia (see Bynon 1977, chs. 6 & 7 for more detail). At present, however, we lack the ability to interpret such typological clustering ('to sort out
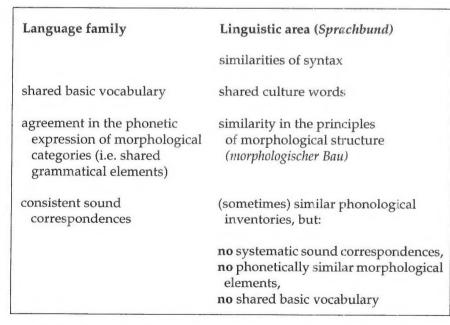
Language family	Linguistic area (*Sprachbund*)
	similarities of syntax
shared basic vocabulary	shared culture words
agreement in the phonetic expression of morphological categories (i.e. shared grammatical elements)	similarity in the principles of morphological structure (*morphologischer Bau*)
consistent sound correspondences	(sometimes) similar phonological inventories, but:
	no systematic sound correspondences, **no** phonetically similar morphological elements, **no** shared basic vocabulary

Figure 1. *Criteria for distinguishing language families and linguistic areas. (From Trubetzkoy 1928.)*

genetic, geographical, and universal determinants of linguistic patterning': Nichols 1992, 1) in an uncontroversial manner. In the case of the Balkans *Sprachbund* we are able to demonstrate from the documented historical record that these languages have all lost the ability to form infinitive constructions and consistently use dependent clauses where their historical relatives outside the area have an infinitive. Thus such structures of Modern Greek, Albanian, Romanian, Bulgarian and Serbian as 'I want that I come' and 'I can that I swim', are demonstrably innovations exclusive to the languages of the Balkans *Sprachbund*.

It will be seen from this rapid review of the analytic means at our disposal that the investigation of prehistoric language involves fundamental decisions about the absolute and relative stability of forms, structures, and typological features that are still unresolved. The comparative method has been found to work well up to the sort of time-depth associated with the classic language families. With greater time-depth, however, it becomes increasingly difficult to verify whether recurrent sound correspondences indicate cognation or pure chance. This is one of the problems posed by such postulated superfamilies as Nostratic, where we have at best short lexical (and perhaps a few grammatical) reconstructed 'roots' of two or three segments, never inflected words to compare. Little wonder then that some historical linguists remain unconvinced (Clauson 1973; Ringe 1995). Secondly, and perhaps more importantly, such 'deep' reconstructions (even if accepted) would be linguistically sterile. They simply do not generate enough substance to work with and the proposed starred forms do not permit us to construct any kind of proto-grammar.

Given that the 'life cycle' of individual words is of limited duration, and that with increasing time-depth even the demonstrable continuity of lexical roots becomes doubtful, linguists of the nineteenth century sought to penetrate further back into prehistory by interpreting the three basic language types — isolating, agglutinating and flectional — as successive stages in the evolution of language. Thus they postulated that certain lexical words of the isolating stage would come to serve grammatical functions and would, in this capacity, attach themselves to some other words (their 'hosts'). In the long run the resulting sequences would fuse to produce the complex word-structures of flectional languages like Greek or Arabic. We have since realized that grammaticalization processes of this kind are taking place all the time, and that it is in this way that languages renew their grammar. But in any one language these processes are not simultaneous and thus no language is in itself typologically pure. (English, for instance, has unanalyzable words of isolating type like *with*, of agglutinating type like *child-less-ness*, and of flectional type like *sang* or *mice*.) Complete cycles of the grammaticalization process from full word to affix to complete fusion are therefore also of limited time-depth. Certain typological features such as head-marking morphology may possibly prove to be more stable (Nichols 1992) but at present this is uncertain.

The one further avenue to be explored, namely the linguistic analysis of place-names, has the undoubted advantage to the prehistorian that the referent is squarely located in geographic space and that, in fortunate cases, places are mentioned in early written sources. Place-names, which include the names of settlements and of geographical features such as mountains and rivers, tend like fossils to survive even total language replacement. Their potential for forming a link between archaeology and linguistics is therefore considerable. The survival of Celtic place-names in the toponymy of Britain, Germany and France, and of Amerindian and Spanish place-names in the United States, is well known.

Of all the classes of toponym which have been studied systematically, that of European hydronyms is perhaps the most significant in this respect, since it allows us to penetrate to a time-depth well beyond the earliest stages of the languages currently spoken in the area. In a large-scale research project entitled *Hydronymia Germaniae* the late Hans Krahe explored the information potential of river-name analysis for the prehistory of Europe (Krahe 1949–64; 1954; 1964). He came to the conclusion that the so-called 'Old European' hydronymy represents the most ancient language material north of the Alps and must on the linguistic evidence be assigned to a language state which, with the exception of one phonological change, is identical with reconstructed Proto-Indo-European.

In view of the fact that onomastics has tended to be the happy hunting ground of etymologizing amateurs, it is important to note that the methodology employed by Krahe and his pupils operates through the systematic analysis of the entire corpus of river-names for a given region. Analysis carried out in this way has invariably revealed chronologically stratified layers. In southern Germany, for instance (Geiger 1963–65), the uppermost layer is German and is largely constituted by smallish rivers with names ending in -*bach* (*Mühlbach* [hundreds of

them], *Buchenbach, Schwarzenbach* — 'mill burn, beech burn, black burn'). The next layer below this is Germanic, represented by compounds with -*ach* (cognate with Latin *aqua*): *Schwarzach, Wolfach,* .... Below this we find single-base (that is, non-compounded) names, generally attached to medium-size rivers: Germanic names such as *Lauter* (AD 772 *riuum Lutra*, from Germanic **hlu:tra-* > German *lauter* 'clear') and Celtic names such as *Pfinz* (1381 *Pfüntz,* <**Puntya, cf.* such Celtic names in Britain as *Pant, Pont,* and Welsh *pant* 'river valley'). These names tend to be identical with, or derivatives of, common nouns that are attested in the Germanic and Celtic languages. Finally, the deepest layer is constituted by names which have direct counterparts in river names found throughout Europe, from Ireland to the Baltic and from Norway to Italy. Thus various Alb rivers of southern Germany, and *Alfenz* in Austria, are etymologically identical with the frequent names *Aube* and *Aubance* in present-day France (< *Alba, Albantia*), and *Alfenz* has a direct counterpart in the Norwegian *Ulvunda* (<*Alfund-* < **albhn̥tya* via typically Germanic sound changes including Verner's Law).

These names of the Old European hydronymy form a morphologically and semantically homogenous system: a dozen or so frequently recurring bases plus a small set of nominal suffixes, most frequently -*na*, -*ma*, -*ra* and -*nt(y)a*. Furthermore the suffixes themselves, as well as the way in which they combine with the base, can (*pace* Vennemann 1994) be accounted for by reference to the nominal morphology of PIE, that is to say they reflect word-formation mechanisms no longer available to early Celtic, early Germanic, etc.. These bases are etymologically associated with words meaning either 'water' or 'flow', or 'white/clear', etc.; that is to say they refer to properties of the water itself and in this respect resemble the most archaic Germanic and Celtic names. Later names, on the contrary, make reference to surrounding flora and fauna and, in the youngest names, refer to human activities, as in *Mühlbach* 'mill-burn'. The typical alternation of -*r*-, -*n*-, and -*nt*- formatives has parallels only in the most ancient strata of the Indo-European vocabulary (*cf.* Sanskrit *vasantá*- 'spring(tide)', and Lithuanian *vasarà* 'summer'; Latin *argentum* and Greek *argyrós* 'silver'; and heteroclitic paradigms such as Hittite *watar* 'water', genitive *wetenaš* etc. (Krahe 1954, 61ff.; 1964, 82–4). Gamkrelidze & Ivanov (1995, 238ff.) relate -*nt*- and -*r*- formatives as active and inactive counterparts.

Britain's place in this ancient European hydronymy can be illustrated by the following examples (Nicolaisen 1972, 94ff.; Krahe 1964, 35–65;

* indicates a reconstructed proto-form; the figures are the dates of the earliest records):

From the base **eis-/ois-/is-* 'to agitate':
*Aisa (from **Oisa*, by 'Old European' **a < *o*) > *Eisa* in Norway and *Jesa* in White Russia;
*Isa > *l'Ise* (1069 *Isa*) and *la Hise* (1113 *Isa*) both in France, *Isa* with place-name *Isaca* in Piedmont, *Ise* (1096 *Isa*) in Brittany;
*Eisia > *Jesia*, and **Aisia* > *Aise*, both in Lithuania;
*Is(a)na > *Iwoine* (943 *Isna*) in Belgium, *Isen* (1096 *Isana*, 8th/9th c. *Isana*) in Austria;
*Eisina > *Eis(bach)* (766 *Isina*) in northern Germany;
Aisontius > *Isonzo* in Italy;
Aisaros > *Esaro* in Italy;
*Eisara > *Ijzer* (1104 *Isera*, 1119 *Isara*) in Flanders;
*Isara > *Isar* (762 *Isura*) in Bavaria, *Isère* (*Isara* Livy etc.) in France, *Aire* tributary of the Ouse in Yorkshire (959 *Yr*), *Ure* tributary of the Swale in Yorkshire (*c.* 1140 *Jor* < **Isura*, with *Isurion*, name of a Roman castellum);

From the base **el-/ol-* 'to flow':
Ala in Norway, Latvia, northern Italy;
*Alva > *Ohle* in northern Germany (earlier *Olua, Alva*); **Alawa* > *Alaw* in Wales, > **Allow* in Cornwall, preserved in the place-name *Porthallow*, 967 *porðalaw*);
*Al(a)venta > *Alwent Beck* trib. of the Tees in Durham (1235 *Alewent*), *Alwin/Alwyn* trib. of the Coquet, Northumberland (1200 *Alewent*), *Allen* trib. of the Tyne, Northumberland (1275 *Alwent*), *Allan Water* trib. of the Tweed, Scotland (12th c. *Aloent, c.* 1160 *Alewent*);
Almos > *Lom* in Bulgaria; *Alma* in Etruria and Norway, > *Alm* in Brabant (1258 *Alme*), > *Yealm* in Devonshire (1309 *Yhalmam*, 1414 *Yalme*); **Almia* > *Elme* in Lithuania, **Almana/Almina* > *Alme* variously in Germany, **Elmana/Elmina* > *Elm* (1346 *Elmone*) in East Prussia, *Ilm, Ilmena(u)* in Germany (1099 *Ilm*); **Almantia* > *Aumance* in France;
Alantia > *Elz* in southern and western Germany, **Alentia* > *Alenza* in Spain;

From the base **neid-/nid-* 'to flow':
*Neida > *Neida* in Lithuania, *Neide* in East Prussia, *Nida* in Poland;
*Nida > *Neth*, old name of the Strat in Cornwall (13th c. *Neth, Net, Nehet*), *Nedd*, English *Neath* in Glamorgan (*c.* 300 *Nido*), *Nidd* in Yorkshire (*c.* 715 *Nid*), *Nethe* in Belgium (1108 *Nita*), *Nied* in western Germany (1028 *Nita*), *Nethe* in eastern Germany (935 *Nithe-*).

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Krahe argued that this Old European hydronymy constitutes the most ancient linguistic stratum in the area which was later to be occupied by Celtic, Germanic, and Baltic. The Mediterranean peninsulae on the other hand have also other naming traditions: in Greece and the Balkans 'Pelasgian' names with -nth- and -ss- formants predominate (*Korinthos, Knossos*); in Italy 'Ligurian' names in -asco and others. As Krahe saw it, the givers of these river-names would have been speakers of a relatively undifferentiated western Indo-European ancestral to the later languages spoken in the area. These later dialect clusters would, in other words, have developed actually within the territory of the hydronymy (see Tovar 1982 for a plausible scenario) and would have included Italic varieties which subsequently moved south. In support of this view Krahe assembled a considerable corpus of lexical isoglosses, including grammatical formatives, uniting these particular branches of Indo-European to the exclusion of all the others. Gamkrelidze & Ivanov (1995, 835–8) accept Krahe's evidence for a late western Indo-European represented in the hydronymy but locate this in the region to the north of the Black Sea, the so-called 'secondary homeland' which they consider to have been an area of dialect levelling and convergence. Italic, Celtic, Germanic and Baltic would each have developed their individuality in the process of migrating to western Europe and they would have brought the 'Old European' names with them. The hydronymy would, in their view, thus constitute a Proto-Indo-European river nomenclature, best preserved in Europe but also attested in traces in southern Russia and above all in Anatolia (Rosenkranz 1966).

On purely linguistic grounds, the hydronymy is thus undoubtedly Proto-Indo-European, but who the people were who actually assigned the names is perhaps less clear. Would speakers of early Celtic and Germanic have remembered and analogically extended an ancient nomenclature that no longer formed part of their active language system? Or would speakers of western Indo-European have reached Europe early enough for the archaic morphology of the hydronymy still to form part of their active grammar? There is some evidence to suggest that the nomenclature may indeed have been extended in a more or less mechanical fashion, so that the river-name base *kar- 'rock' which extends to Mediterranean Europe may indicate the absorption of a non-Indo-European lexical element (Tovar 1982). There is also some evidence that certain vowel alternants (for example *is- rather than *ois-; see above) may have been generalized locally. Both types

of development have parallels elsewhere. Thus *Juliacum > Jülich* combines the Roman personal name *Julius* with Celtic -(i)acum, and the elements of the Germanic personal naming system of the type *Winifred* and *Ger-trud* appear to have been combined freely in late Germanic, irrespective of the meaning of the individual bases.

Linguistic Evidence for Human Prehistory

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Today no linguist doubts that the evidence of comparative linguistics can contribute to our understanding of human prehistory. Indeed, the discovery and reconstruction of Proto-Indo-European (PIE) over the past two centuries is generally regarded as the crowning achievement of historical linguistics. Yet at the same time most linguists have come to believe that the Indo-European family represents the limits of comparative linguistics beyond which linguistic evidence can shed no light. Were this true, comparative linguistics would be of little value in the study of human prehistory, most of which transpired before the alleged date of PIE, 4000 BC. Furthermore, it would mean that we could never discover Indo-European's closest relatives, nor indeed the relatives of any of the myriad language families whose time depth is comparable to that of PIE.

Such ideas have held sway throughout most of this century, despite the protests of a small group of dissenters. In the past decade these questions have once again emerged as a major focus of debate. The alleged isolation of Indo-European has once more been vigorously challenged, as have the hundreds of independent families claimed for the Americas, and even the question of the possibility of monogenesis of all extant languages — virtually a taboo subject within historical linguistics — has once again been broached. What has perhaps changed most in recent years has been the interest of scholars outside linguistics. Geneticists have come to see that the structure of the human population based on gene frequencies parallels to a remarkable extent that based on language — including proposed linguistic families that go far beyond the time period of PIE (Cavalli-Sforza *et al.* 1994). Archaeologists, for their

part, have seen that this emerging picture of human prehistory is not incompatible with the archaeological record, which may itself clarify certain issues left unresolved by the study of human genes and languages (Renfrew 1991; 1992b).

The comparative method

In my view the source of the current controversies over the alleged limits of comparative linguistics is a fundamental misunderstanding of the nature of the comparative method, which comprises two stages: (1) taxonomy, (2) historical linguistics. The failure to distinguish these two stages — or even to consider them one and the same thing — underlies most of the current disputes.

The first step in the comparative method must be to group together languages sharing similar words. This stage is variously called taxonomy, classification, mass comparison, or multilateral comparison. Consideration of just a dozen words or so will permit even an archaeologist with no linguistic expertise to segregate Romance, Germanic, and Slavic languages from one another in three groups. The evolutionary hypothesis that explains the similarities defining each family dates back over two centuries to the well-known observations of Sir William Jones. Consideration of a broader context also quickly reveals that these three families have certain words in common that distinguish them as a group from Semitic, Bantu, or Algonquian languages. It was on this basis that Jones identified the Indo-European family in 1786.

The second step in the comparative method is the investigation of a language family identified during the first stage, be it Romance, Indo-European, or any other valid linguistic taxon. It is this stage that is generally known as 'historical linguistics'. Its task is seen as the reconstruction of the proto-language, the discovery of regular sound correspondences among the constituent languages or branches, the search for the original homeland and the time at which this proto-language existed, etc.. It should be clear that all these questions presuppose a previously-identified language family.

For a number of reasons, which are discussed elsewhere (Ruhlen 1994a,b), historical linguists have come to view taxonomy as an unwelcome alternative to — rather than a requirement for — historical linguistics. Thus, for example, Theodora Bynon (1977, 272), in a discussion of Joseph Greenberg's method of multilateral comparison, writes that 'as far as the historical linguist is concerned, it [multilateral com-

parison] can in no way serve as a substitute for reconstruction, for to him the mere fact of relationship is of little interest in itself'. Yet Greenberg has never proposed that taxonomy is a *substitute* for reconstruction, and he has on more than one occasion pointed out that when he does reconstruction he proceeds just like anyone else. What has happened is just the reverse: historical linguists have somehow come to see reconstruction as a *substitute* for taxonomy, something it assuredly is not.

Indo-European relatives

The question of whether Indo-European has any relatives is, however, not a methodological issue, but an empirical one. It cannot be decided by *a priori* speculation. What if one were to compare Indo-European with surrounding families in Eurasia and Africa, just as one has compared Romance, Germanic, and Slavic? Would Indo-European turn out to be closer to some families than to others? Or would there be no perceptible evidence connecting Indo-European with any other family? Ironically, the answer is that Indo-European's closest relatives are obvious — and have been for a hundred years — yet most historical linguists still consider Indo-European to have no known relatives. At the start of this century, Alfredo Trombetti (1905, 44) noted the ambivalent attitude of historical linguists to evidence:

> It is clear that in and of itself the comparison of Finno-Ugric *me* 'I', *te* 'you' with Indo-European *me-* and *te-* [with the same meaning] is worth just as much as any comparison one might make between the corresponding pronominal forms in the Indo-European languages. The only difference is that the common origin of the Indo-European languages is accepted, while the connection between Indo-European and Finno-Ugric is denied. (Trombetti 1905, 44)

And is still denied to the present day by most historical linguists.

The M/T 'I/you' pronoun pattern is now known to characterize not just Indo-European and Finno-Ugric (= Uralic), but also Altaic, Gilyak, Chukchi-Kamchatkan, and Eskimo-Aleut in a family that Greenberg calls Eurasiatic (Korean, Japanese, and Ainu are also included). A somewhat expanded version of this family, called Nostratic by Russian scholars, adds Dravidian, Kartvelian, and Afro-Asiatic to Greenberg's Eurasiatic grouping, though only Proto-Kartvelian *m-* 'I' resembles the Eurasiatic pronoun system.

As powerful as the evidence of the M/T pronoun pattern is for ancient historical relationships (these pronouns are known to be extremely stable

over long periods of time), Greenberg (forthcoming) has discovered even more compelling evidence in the presence of the same suppletive alternation for the first-person pronoun in both the Indo-European and Chukchi-Kamchatkan families, located at the extremes of Eurasia. A hallmark of PIE is that the subject form and object form of the first-person pronoun are based on different roots: *eg(h)om 'I' and *me 'me', a suppletion preserved in English 'I/me', French je/moi, Russian ya/menya, etc.. An identical suppletion is found in the southern dialect of Kamchadal in the Chukchi-Kamchatkan family: kim 'I' and ma 'me'. In Chukchi itself we find not only -i-γə-m 'I', but also -i-γə-t 'you'. The suppletive object form has not, however, been preserved. In addition to these two families Greenberg has also found traces of this system in Uralic (e.g. Kamassian i-gä-m 'I am', Hungarian en-ge-m 'I') and Eskimo-Aleut (Eskimo kə-t 'thee'). In reality the first- and second-person pronouns are sufficient to identify Indo-European's closest relatives, as Trombetti realized. Yet this single example represents but a tiny fragment of the evidence that has been adduced by the Nostraticists (Illich-Svitych 1971–84; Bomhard & Kerns 1994) in support of the Nostratic family. In my opinion this already published evidence is more than sufficient to identify Indo-European's closest kin.

Native American languages

From the perspective of the archaeological record the Americas represent a puzzle. According to specialists in Native American languages there are over 200 independent families in the Americas, without visible connections among them. In Africa, by contrast, there are but four linguistic families recognized by Africanists (Greenberg 1963). The puzzle is how an area of the world which shows virtually no trace of any hominid until the appearance of modern humans around 12,500 BP could have produced such bewildering linguistic diversity in so short a time, while Africa, where most of human evolution seems to have taken place over tens of thousands of years, shows so little diversity. Greenberg's (1987) solution to this dilemma — the classification of all Native American languages into just three families — has provoked a storm of controversy in recent years. Of the three families posited by Greenberg — Eskimo-Aleut, Na-Dene, and Amerind — only Amerind is controversial. This vast family encompasses most of the languages of North America and all of those in South America.

Part of the evidence that Greenberg offered in support of Amerind was a distinctive pronoun pattern, N/M 'I/you', occurrences of which he documented in abundance throughout North and South America. Ironically, the first person to adduce evidence of this pattern in the Americas was Trombetti (1905) who devoted an entire appendix to evidence of this particular pronoun pattern. A decade later Edward Sapir, apparently independently of Trombetti, noted the same pattern. Subsequently, Swadesh, Greenberg, and others have stressed the prevalence of this pronoun pattern in the Americas.

Since the publication of Greenberg's book a particularly telling lexical item has been discovered that connects Amerind languages no less than its distinctive N/M pronoun pattern. As argued in Ruhlen (1994c), Proto-Amerind appears to have possessed a kinship term that signalled the sex of the referent by the first vowel in the root: *t'ina 'son, brother', *t'una 'daughter, sister', *t'ana 'child, sibling'. Numerous traces of these three morphologically-related forms are found in dozens of Amerind languages of North and South America. An attempt has also been made recently to work out the Proto-Amerind system of counting, with some interesting results (Ruhlen 1995).

It should also be noted that Cavalli-Sforza and colleagues (1994) discovered essentially the same three populations in the Americas on the basis of gene frequencies that Greenberg had posited from language. In addition, Christy Turner (1989) has postulated the same three groups on the basis of dental morphology. That the linguistic, genetic, and dental evidence all support a three-way classification of aboriginal American populations — the same three-way classification — can hardly be accidental. The possibility that there were exactly three migrations from Asia to the Americas, as proposed by Greenberg et al. (1986), would seem to be quite likely, and such a hypothesis carries major archaeological implications.

The global picture

The denial that Indo-European is related to any other family — the central myth of twentieth-century historical linguistics — has effectively precluded a comprehensive classification of the world's languages. Rather, historical linguists have become comfortable with the notion that the world contains hundreds of independent families, any genetic connections among which have long since disappeared. But what if we followed the comparative method to its logical conclusion, without preconception or prejudice, and compared the dozen large families that have now

been posited for the entire world: Eurasiatic, Amerind, Dene-Caucasian, Niger-Kordofanian, Australian, etc. (Ruhlen 1991). Could such ancient and geographically far-flung language families themselves harbour clues of an earlier unity? The answer was given by none other than Trombetti (1905) in the same book in which he noted both the M/T pronominal pattern in Eurasia and the N/M pronominal pattern in the Americas. In fact, the answer to this question was given in the title of the book itself: *L'unità d'origine del linguaggio.*

Recently John Bengtson and I (Bengtson & Ruhlen 1994) — building on the work of notable precursors going back to Trombetti — have proposed some two dozen roots that seem to enjoy a broad distribution among the world's dozen or so language families. We invite each reader to examine the evidence we present and to decide for himself whether these are just 'accidental' resemblances, as critics would have it. But what if Bengtson and I are right, and the linguistic similarities we have uncovered really do represent traces of a single earlier language family? What would be the implications for archaeology — and human prehistory — of this fact? First of all, it must indicate that the origin of *present* linguistic diversity is fairly recent, otherwise similar words in many different families would simply not be found. But what is 'fairly recent'? If Indo-European can be distinguished by anyone on the basis of a dozen words — and no one seems to doubt this — and if Indo-European is 6000 to 8000 years old, depending on whose views of the origin of Indo-European one subscribes to — is it then too much to expect that trained linguists, through a careful sifting of all the world's linguistic evidence, might not be able to extend the linguistic horizon four or five times beyond the obvious? What seems to me the most probable explanation of the linguistic data, as they are presently known, is that current linguistic diversity derives from the appearance of behaviourally-modern people forty or fifty thousand years ago. While anatomically-modern humans may have appeared in Africa before 100,000 BP, these people did not behave like us. That in itself may indicate their more rudimentary linguistic skills. Several scholars have in fact suggested that the 'sapiens explosion', as it is sometimes called, involved the development of fully modern human language (Klein 1992; Diamond 1992) as recently as 40,000 years ago (or perhaps somewhat before). It seems to me that the serious research on this question — stretching from Bengtson and myself back to Trombetti — has always supported such a conclusion. The irony has

been that it has always been linguists who have blocked the exploration of the linguistic record, with assurances that 'there is nothing there'. Perhaps the time has finally come for a more sober appraisal of the linguistic record, where the data are finally allowed to speak for themselves.

Linguistic Prehistory

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Is linguistic prehistory possible? In answering this question, let us first define 'prehistory'. By prehistory we usually mean part of history earlier than any written sources.

If we understand 'prehistory' in this way, we will be able to say that the concept of 'linguistic prehistory' comprises: (1) reconstruction of languages unattested by script or scholarly records, (2) valuable information provided by linguistics for the reconstruction of the prehistory of peoples, and (3) any information helpful in solving the problem of the origin of human language, as well as the problem of its monogenesis or polygenesis.

1. Reconstruction of languages

Linguistic science in the nineteenth and twentieth centuries has elaborated methods and technique for the partial reconstruction of ancient languages based on the evidence of their later stages (descendant languages). Many proto-languages, such as Proto-Indo-European, Proto-Semitic, Proto-Uralic, etc., have been partially reconstructed. Today scholars try to reach a deeper level of reconstruction. By comparing languages of the Nostratic macro-family (> Indo-European, Hamito-Semitic [Afroasiatic], Kartvelian [South Caucasian], Uralic, Altaic, Dravidian, as well as Elamite, Chukchi-Kamchatkan, Eskimo-Aleut, and certain other families and languages) we try to approach a reconstruction of Proto-Nostratic, a common ancestor of the above-mentioned languages which existed about 15,000 years ago or thereabouts. The late V. Illich-Svitych (1934–66) started reconstructing Proto-Nostratic in the 1960s; his etymological research is being continued by the present writer. Our aim is to reconstruct the phonological and grammatical structure of Proto-Nostratic and its

lexical stock. In the file of my Nostratic Dictionary (in prep.) I have more than 2000 Proto-Nostratic words (roots). Other scholars have begun to reconstruct proto-languages of other macro-families, such as Sino-Caucasian (S. Starostin, S. Nikolayev, J. Bengston, and others). Some are attempting to reach an even deeper level of reconstruction, as far back as to what they believe to be 'Proto-Global'.

Serious reconstruction of our linguistic past is based on the establishment of strict phonological correspondences and laws of historical phonology for the language families in question. Successful reconstruction of a deeper level therefore depends on the existence of reliable reconstructions in later levels. For instance, reconstruction of Proto-Altaic is based on a good reconstruction of Proto-Turkic, Proto-Mongolian and Proto-Manchu-Tungusian. Today the conditions are ripe for a reconstruction of Proto-Nostratic, since very serious etymological research has been accomplished on the descendant languages (Indo-European, Semitic, Uralic, Kartvelian, Dravidian, Altaic with its branches) so that we have a solid basis for a deep (Proto-Nostratic) reconstruction. In the comparative linguistics of Sino-Caucasian (or Dene-Caucasian) much is being done to reach a good reconstruction of daughter-families (North Caucasian, Sino-Tibetan). This is a necessary premise for a sound reconstruction at the macro-family level. But in other linguistic macro-families the situation is still not so bright: the reconstructive work on Niger-Congo, Nilo-Saharan, Amerindian, and Australian (macro-)families is at its initial stage. It is clear that sound reconstructive research at even deeper levels ('Proto-Global') can only be carried out successfully if we have at our disposal satisfactory reconstructions of every language family of the world. But this does not mean that preliminary attempts at reaching back to 'Proto-Global' cannot be useful even today.

2. Historical information based on linguistic data

The usefulness of linguistic data for ethnogenesis is obvious. The very fact that Hungarian is closely related to Ostyak and Vogul (languages of ethnic groups which in the eleventh century inhabited a region to the west of the Urals) suggests a habitat of ancient Hungarians in the same or neighbouring region. Etymological research proves that in the Hungarian lexical stock there are loans from Old Bulghar (the Kama region, to the west of the Urals), from Alan (a medieval Iranian language in the steppes to the north of the Caucasus) and from Turkic languages. This fact provides us with important information on the route of migration of the Hungarians from the Urals to Pannonia and on their cultural background.

Another example is the search for the Indo-European homeland. The presence of some words of Semitic origin in Proto-Indo-European (such as IE *septṃ 'seven' < Semitic *šabfatum 'seven', IE *haster- 'star' < Semitic *faθtar- 'morning star, Venus', IE 'bull' < Semitic *θauro-, etc.) suggests that the speakers of early Indo-European were neighbours of the Semites. This (together with other arguments) leads us to a hypothesis of an Indo-European homeland in Asia Minor. These Semitic loanwords and the absence of cultural contacts between Proto-Indo-European and Proto-Uralian (located to the north of the Caspian Sea) rule out the possibility of locating Proto-Indo-European in the Ponto-Caspian steppes (South Russia and Ukraine), as conjectured by Marija Gimbutas and her school. Linguistic data suggest rather that speakers of Proto-Indo-European moved from Asia Minor to the Balkan Peninsula, and that later the Indo-European languages spread throughout Europe and moved to Central Asia, Iran and India. This hypothesis may be compared with the similar results of Colin Renfrew's research based on archaeological sources.

Another source of historical information is so-called 'linguistic palaeontology', i.e. data provided by meanings of words in a proto-language. A careful and critical analysis of the lexical stock of a proto-language can suggest both geographical and cultural parameters of the corresponding linguistic community. Thus in Proto-Indo-European there are many words associated with agriculture and husbandry, suggesting that Proto-Indo-Europeans were a Neolithic people with a food-producing economy. On the other hand, among the 2000 roots of the Proto-Nostratic lexical stock we do not find words suggesting acquaintance with agriculture or husbandry, but we do find many terms associated with hunting and food-gathering. If we take into account the fact that in other descendant proto-languages within the Nostratic family (Proto-Dravidian, Proto-Kartvelian, Proto-Altaic, Proto-Semitic, etc.) there is a rich terminology for agriculture and husbandry, we may suggest that Proto-Nostratic belongs to the period prior to the 'Neolithic revolution', while most of its descendant languages belong to the Neolithic epoch of food-producing economy. This is an important indication which may help us to suggest to historians and archaeologists where the Nostratic language might have been spoken. Another argument is the very location of the descendant languages:

early Proto-Indo-European in Asia Minor, Proto-Hamito-Semitic in the Near East (and northeast Africa?), Proto-Dravidian probably somewhere in Persia, Proto-Kartvelian in Transcaucasia, Proto-Uralic and Proto-Altaic probably in Turkestan and adjacent regions. On the other hand, we know that the most ancient centre of Neolithic economy in western Eurasia was situated in southwest Asia. All this leads to a preliminary hypothesis that Proto-Nostratic was spoken in southwest Asia at a period prior to the 'Neolithic revolution', while most of its daughter-languages belong to the Neolithic epoch, and their spread over large territories of Eurasia and Africa was connected with the demographic explosion caused by the 'Neolithic revolution'. Of course, this is only a hypothesis suggested to archaeologists and proto-historians. Only by the joint efforts of archaeologists, proto-historians, and linguists, together possibly with anthropologists and geneticists, can we hope to achieve the truth.

There are many examples of such historical conclusions based on analysis of the lexical stock of proto-languages. Pelio Fronzaroli has shown that Proto-Semitic possessed a rich terminology concerning agriculture, horticulture and viticulture, which invalidates the traditional hypothesis of the Arabian Peninsula as the home of the Proto-Semites. The presence of words for 'hedgehog', 'elm (*Ulmus*)' and 'Siberian cedar (*Pinus sibrica*)' in Proto-Finno-Ugrian, on the other hand, leads scholars to the conclusion that the ancient Finno-Ugrians inhabited the region of the Urals, because from palaeozoology and palaeobotany we know that there were no hedgehogs and elms to the east of the Urals and no Siberian cedar trees to the west of them.

3. The origin of language. Monogenesis or polygenesis?

Even if scholars prove that all known (attested and reconstructed) languages are akin, and even if they reconstruct their common ancestor, it cannot be claimed that they have reached the real Proto-Global. The trouble is that a great many (maybe most) prehistoric languages have disappeared without leaving descendants or any other traces. What do we know about the languages of the numerous pre-Indo-European populations of Europe (those of Aurignacians, Maglemosians, etc.), or about the pre-Berber languages of North Africa, the pre-Altaic peoples of Siberia, and so on? We know nothing even about the lexicon and grammar of the languages of many ethnic groups and tribes mentioned in Greek, Latin,

Egyptian, Assyro-Babylonian, Hebrew and Chinese literary sources: Ligurians in Italy, Rhetians in the Alps, Lullubi in Iran, etc., etc..

Hence comparative linguistics is unable to prove the monogenesis of the human language, even if it is proven that all known languages are related and form one super-macro-family.

There is nevertheless a speculative argument favouring the hypothesis of a monogenetic origin of language. It is based on two universal qualities of language:

(1) In the history of all attested and reconstructed languages we do not find *creatio ex nihilo*. All words (save some onomatopoeic exceptions) are either inherited from earlier stages of the same language, derived from other words, or borrowed from other languages. All grammatical morphemes go back to something (either earlier morphemes, or words, or their parts undergoing a metanalysis).

(2) Any language is a self-organizing system, having the ability to adapt itself to communicative needs of the speech community. To quote only one example: Mongolian, which up to the sixteenth century AD was a language of primitive nomadic horse-breeders and hunters, later became the carrier of a highly sophisticated Buddhist culture. There are hundreds of volumes of Buddhist literature in Mongolian. Terms belonging to Buddhist philosophy, theology, psychology, etc., were formed mainly using inherited roots and derivational structures rather than from Tibetan or Sanskrit loans. In the twentieth century, Mongolian adapted very quickly to the communicative needs of a modern society — again mainly with its own sources. Such examples are numerous.

If we may extrapolate these qualities of language into the distant past, we shall have to conclude that the language of *Homo sapiens* cannot have been created *ex nihilo*: it must go back to communicative systems of earlier species of *Homo* and eventually to those of pre-human hominoids. The immense gap between the poor communicative systems of primates (as observed today by biologists) and the rich language of humans does not prevent us from bridging the gap, if we take into account the second aforementioned universal quality.

Now, if we accept the monogenetic hypothesis of the origin of *Homo sapiens*, we have to hypothesize a monogenesis of human language too.

Of course, these speculations and extrapolations are not a decisive argument in solving the problem of the origin of language and of its mono- or

polygenetic source. But they provide one of the arguments which should be taken into account in our common effort to unveil the secrets of the prehistory of human speech.

What linguistic science *cannot* do today (as far as I know) is to approach a reconstruction of the communicative systems of *Homo habilis, Homo erectus*, etc.. I wonder if palaeoanthropological research could be helpful here, perhaps in determining the articulatory and cerebral abilities of earlier humans by studying their skulls and other fossil remains?

Language Families and Human Dispersal

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Every time we talk to someone we present a signal, for those able to interpret it, about our place and that of our ancestors in the pattern of human cultural history. The purpose of this brief note is to present a significant observation about the factors which have given rise to this pattern, an observation derived from the history of human languages at the macroscale family level. Before doing this, I should perhaps state my credentials. As an archaeologist, my own detailed experience of linguistic methodology has been mainly in the field of Austronesian studies, a field focused on many of the peoples of Island Southeast Asia and Oceania. This region, with its somewhat 'laboratory-like' situation of many islands, some rather isolated, has produced a huge linguistic literature within which some major successes in methodology and historical interpretation have occurred. Some idea of how comparative historical linguistics can illustrate aspects of the prehistory of a population whose descendants today number some 270 million people, all speakers of phylogenetically related languages within the Austronesian family, can be obtained from a number of recent general surveys (Blust 1984–85; Blust in press; Pawley & Ross 1993; Bellwood *et al*. 1995). It would be an understatement to suggest that these linguistic reconstructions are merely adjuncts to an understanding of the archaeological record of Southeast Asia and Oceania; I personally find it impossible to conceive of a meaningful reconstruction of the cultural aspects of human prehistory during the past few millennia which does not include a substantial amount of linguistic input.

It would be like writing a history of the early English devoid of any reference to the origin and history of the language.

My purpose here, however, is not to extol the virtues of linguistic prehistory for the benefit of the dubious archaeologist. There are pitfalls in attempting to equate the results of the two disciplines, as most of us are probably aware. Obviously, we can never know the language spoken by the person who made *this* Neolithic pot or *that* Bronze Age axe. Obviously, many peoples through history have undergone the experience of language shift, whereby languages, phenotypes and cultures develop in situations of relative disjunction. The existence of such situations (which are by no means universal) does not mean that linguists and archaeologists cannot aid each others' reconstructions of prehistory. On the positive side, we can look at and compare patterns in language history and archaeology, both in time and space. We can also look, in order to develop explanatory models, at how languages and their speakers have behaved in history, through the immense record of empires and ethnography and into the modern world as seen through the lens of sociolinguistics. The fields of sociolinguistics and language contact studies become very important in informing successful explanations of language family genesis and expansion.

One of the most important observations to be made from linguistic data, an observation with significant archaeological ramifications, derives from a consideration of large-scale language family history. This, put simply, is that those language families which have attained great geographical extents, such as Indo-European, Afroasiatic, Austroasiatic, Austronesian, Uto-Aztecan, Algonquian, Athabaskan, Dravidian and many more, can only have spread *predominantly* (but certainly not entirely) by population expansion; that is by an outwards movement, from a homeland region, of speakers of early forms of the descendant languages which comprise each of the families today. Some archaeologists naturally refuse to accept such an interpretation and, when faced with questions of how language families have spread, take anti-migrationist refuge under the warm and snug defences of bilingualism, lingua francas, élite dominance, in-marrying males or females, pidginization and even a rather mysterious concept called 'convergence'. Convergence, perhaps better known to linguists as 'areal convergence', does of course occur in language history (good examples are recorded for South Asia, Mesoamerica and Australia) as a result of bilingualism and intensive borrowing.

It can serve to reduce the lexical and grammatical distinctions between neighbouring languages, thus assisting multilingualism, but there is absolutely no evidence that convergence alone could produce a family of phylogenetically-related languages out of an array of unrelated babels across the geographical scale of any one of the major families. No linguist has suggested such a scenario on a *language family scale* since the issue was raised by Trubetzkoy, rather briefly and certainly not very convincingly, for Indo-European in 1939.

The record of population expansion enshrined in major language family distributions appears to be accepted by most linguists but rather ignored in the other anthropological sciences. For instance, many anthropologists today, particularly those well informed about the history and politics of recent decades, favour a view of cultural evolution which stresses a kind of interactionary process between neighbouring groups, a process which one could term reticulate, rhizotic (Moore 1994a) or osmotic. Cultural boundaries are considered to be fluid, with constant mixing and reformulation of cultural and linguistic entities. No ethnic patterning exists for more than a fleeting moment in time. Morton Fried gave an early exposition of this view in 1975. In similar vein, Rowlands (1994) rejects the concept of unique natural cultures and favours a concept of continuous creolization to explain cultural patterning through time. Moore (1994a,b) has published a detailed exposition of the same view in which he states that such 'rhizotic' processes are the only sources of ethnogenesis and that 'bifurcatory' (or cladistic/ branching) processes have no place in anthropological or archaeological interpretation. Moore also adopts the uniformitarian stance that the past can only be understood according to historical processes witnessed in the ethnographic record. But of course, apart from a few individual subgroups such as the Romance languages, history has never revealed an actual language *family* expanding; we only witness individual languages engaged in this process. Our written records are simply too brief in time and restricted in space. From his uniformitarian viewpoint Moore therefore suggests that hypotheses of population expansion connected with language family and macro-family reconstructions, such as Nostratic, are theoretically unsound. They remain unwitnessed.

I wish to make it very clear here, before moving to specifics, that I do not exactly disagree with the authors I have just mentioned; I simply think they have not considered all the options, especially those which are relatively undeniable on the linguistic

macro-scale. In this regard, scale is an *essential* factor. If one looks at the ethnographic world on the scale of neighbouring groups of communities (i.e. the micro-scale) one sees the results of reticulate interaction everywhere. I know this well myself since my own current archaeological fieldwork is in an area of eastern Indonesia (the northern Moluccas) where biology, culture and language have clearly marched in different directions in the recent past. Moore's example of rhizotic interaction between Great Plains Indian communities in the nineteenth century and Upham's (1994) similar interaction-based model of nomad-settled interaction in the 'Desert West' of the USA make excellent sense. A lot of ethnogenesis surely happened this way.

But a different viewpoint must emerge, in part, if one thinks carefully about the historical significance of linguistic families or phyla on the scales of Indo-European or Austronesian. This is the macro-scale — a scale of thousands of communities whose languages reflect patterns which are too vast to be the result of intercommunity 'creolization'. A few points need to be stressed here. Firstly, these large-scale language families are well-defined in terms of structure and lexicon, to the extent that very few languages within their orbits are considered genuinely non-classifiable. Pre-colonial pidgins and creoles are quite rare on a world scale, an observation which applies even to regions of widespread contact-induced change such as Melanesia (Dutton 1995, 201–2). The world is not an even gradation of linguistic variation and language families differ from each other coherently and often meet each other with quite sharp boundaries. In other words, they are taxonomically real, an observation supported by internal observations of shared phonology, lexicon and aspects of syntax which cannot simply be explained away through borrowing. Furthermore, and this is a major point, most of these families had attained geographical distributions very close to their ultimate extents before the occurrence of any written history. This is true even if one considers historical expansions of languages such as Latin, Turkish, Chinese and Malay. All of these languages cover(ed) quite small areas compared to the total language families to which they belong. English and Spanish after 1492 have obviously done much better on a geographical scale, but this is as a result of the most intensive period of colonization in world history. English and Spanish did not for the most part 'diffuse' or spread by language shift. They spread initially with native speakers, despite large doses of subsequent adoption by other groups.

The written records of language history are important here because we can see that conquest without colonization has been relatively insignificant on the macro-scale for language replacement. As an example, to judge from the development of the Romance languages the Roman Empire only managed to spread Latin as a true vernacular replacing older vernaculars (and not just a language of administration) over about one-third of the empire, despite 400 years of imperial control. Latin was probably spread to the provinces mainly by retired speakers of Latin, who mostly favoured the inner regions of the empire for settlement (Brosnahan 1963). Conversely, English and Spanish in recent centuries have been spread mainly by actual colonization into Australia and the Americas. The aboriginal populations of these regions, even after several centuries of outsider domination, still keep their native languages where social networks and population numbers allow. This is overwhelmingly true of countries such as India and Indonesia where European political domination occurred but where actual colonization could not occur for cultural and demographic reasons. We have very few examples in history of any spread of language, including even Arabic (Lewis 1977, 176), on anything approaching a macro-scale without a substantial component of actual population movement (Bellwood in press a). Obviously, written languages and the attractions of charismatic world religions can provide much inducement for language shift to a high status target language, but for prehistoric societies such inducements either would not have existed or would have been very localized.

All of this means that the major language families can only have spread to their prehistoric limits by means *predominantly* of population movement, although there is not space here to enter into discussion of important questions such as the historical determinants and speeds of these movements. I would stress, however, that there is always scope for reticulate interaction as well, in the vast majority of times and places. But those who favour the ethnolinguistically reticulate course of ethnogenesis as the only possible one may have to accept that the prehistoric past was not always quite like the shrinking and rather reticulate ethnographic world of the last few centuries. Even here there have been exceptions to this norm, such as the Iban of Borneo, who have combined both relatively large (but certainly not continental) scale expansion and interaction with other groups in their recent history. From a language family perspective, such punctuational expansions of languages and speakers of varying geographical scale

must surely have separated long periods of reticulate interaction.

In other papers (Bellwood 1994; in press b,c) I have approached the question of population expansion as a result of the establishment of agricultural economies, in similar vein to Colin Renfrew (1987; 1992a,b; 1994) but with a focus on different regions of the world (mainly Asia-Pacific). As it happens I am a firm supporter of Renfrew's Neolithic expansion hypothesis for initial stages of Indo-European, but of course early agricultural expansion is not the whole story for this or all other language families. Some hunter-gatherers have colonized in recent millennia too (Athabaskans, Eskimo-Aleuts and perhaps Pama-Nyungan speaking Australians). Furthermore, not all the language families of agriculturalists necessarily reached their full extents during periods of early agriculture, an observation recently re-stressed for Indo-European by Mallory (1994). Some very major language expansions have occurred in recorded historical time all over the world. All I wish to stress from my own perspective is that we should look at early developments of *systematic* agriculture (and this may be a different concept from that of incipient agriculture) as potential triggers for some very major population and language dispersals, dispersals for which the recent colonial expansions of English and Spanish speakers may be quite appropriate for comparative purposes. What will English have evolved into by the year AD 4000? Another major subgroup of Indo-European? Communications and mass population relocation may make the linguistic future of humanity a different kind of ball game from the preliterate past, but the question is worth asking.

By the same token, it is also worth asking what the historical connotations of linguistic macrofamilies like Nostratic and Austric must be. My own view certainly favours, as for individual language families, a very high degree of population dispersal fuelled by demographic growth from homeland regions of relatively restricted geographical extent. Early agriculturalists and early hunters alike, if offered opportunities to colonize into empty (or relatively uncontested) yet suitable environments, could have benefited from circumstances, albeit perhaps short-lived ones, of good health and considerable fecundity. History tells us such situations are unlikely to have lasted for long, but that is another story.

One conclusion I would offer is that interpretations of human history and prehistory cannot proceed solely according to uniformitarian models based

on the ethnographic record. Historical contingency matters, and every conjunction of circumstances is unique. Human history unfolds in a highly stochastic way and nothing which seems unusual from an ethnographic viewpoint need be deemed impossible, even periodic and relatively rapid episodes of geographically-extensive ethnolinguistic dispersal. Reticulate processes of ethnogenesis occur continuously wherever different societies interact, but not always with a uniform intensity. The record of prehistory reflects as much the human concern with ancestry and identity as it reflects the results of inter-population interaction. Careful consideration of the patterns enshrined in the continental-scale records of archaeology, linguistics and biological anthropology render any more restricted conclusion inherently unlikely.

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